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PROGRESSIVE PARALYSIS OF THE INSANE.

(PARALYSIE GÉNÉRALE DES ALIÉNÉS OF THE FRENCH.)

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(Concluded from page 892.)

DIAGNOSIS.

It is the imperious duty of the careful physician not to make too suddenly his conclusions. Nothing is more annoying than to learn that our judgment was erroneous. Nothing more embarrassing than to be able to early distinguish this sad, fatal disease from some other forms of mental derangement. In cases of doubt, then, let us learn not to be too hasty.

Usually the most difficulty will be presented in distinguishing general paralysis from that form of insanity known as simple mania, when, especially, assuming an ambitious character. As soon as we find a patient divulging ambitious delirium, we must be on our guard, remembering that this tendency is, in the immense majority of cases, closely connected with lesions of motility. All authors usually admit that mania of an ambitious type may exist independently of being

connected with those terrible indications which truly reveal to us the fatal termination of general palsy.

Consequently, if our patient, with his agitation, irascibility, and ambitious ideas, does not present impediment of speech, or trouble of motility, we must carefully become informed concerning his antecedents and the circumstances which preceded the beginning of his disease. By thus searching, we shall find, if we have to do with a case of general palsy, that before the manifestation of ambitious delirium, memory was diminished; and, in fact, all the symptoms of intellectual weakness may be recognized. Especially should we ascertain if the patient has been subject to cerebral congestion, followed by difficulty in speaking.

M. Calmeil says: "We must not hasten to pronounce a diagnosis from a simple impediment of speech with or without delirium, because this may possibly arise from a deep inflammatory focus with or without a clot of blood in the cerebral substance." Ambitious monomania of the paralytic is easily distinguished from ordinary ambitious monomania. This latter is rare; the person affected with it, taciturn and morose at the beginning, realizes that he is ill; fears that he is going to lose his reason; and, at first, endeavors to banish the delirious ideas which he knows to be absurd. Later, mastered by the delirium, he conforms himself to his ideas; he imitates the character or person he has chosen; his actions and words seem consistent with the position he believes himself called to fulfill. If he considers himself general, or king, then does he use terms suitable for his illusory dignity.

Hallucinations, rare with the paralytic, are, on the contrary, common to him.

Nothing similar is seen with the victim of general palsy. His ideas are not only absurd, but they are very changeable. He, while king, and at the same moment Pope or President, speaking of his millions, will acknowledge his profession, however humble it may be, and declare he has only a few cents in the world.

The ordinary ambitious monomaniacs retain for years their

systematically organized delirium, while the paralytic is continually, day by day, hour by hour, changing his absurd ideas. Finally, the absence or presence of trouble of motility, the whole course of the two diseases, are too well known to be here longer considered.

To distinguish general palsy from simple dementia, we must remember that this latter impaired mental condition occurs consecutive to some other cerebral affection, existing even for years, as from melancholia, or mania; and that by slow gradations the mental faculties have become confused, and at length obliterated. Although here we may find abolition, more or less apparent, of the intellectual, sensitive, and voluntary faculties, yet when we recall to mind that paralytical dementia has not been preceded by any mental affection; that in a few weeks, or a few months, it is so apparent as to be recognized by slight observation; that without delay there is the appearance of delirious ideas, either ambitious or melancholic, we hardly need to be obliged to base the diagnosis on the presence or absence of difficulty of speech, or trouble of motility.

In speaking of insanity, Dr. Aitken says that melancholia comes next in frequency to mania, and that the melancholic and stupid are most frequently cured or improved. Such being the case, it behooves the physician not to confound the appalling disease of general palsy of a melancholical form with melancholia of a stupid nature. To well found the diagnosis in some cases is often perplexing, as the delirious ideas are the same; dementia in both often very similar; and the stupor is accompanied with trouble of motility. We must carefully ascertain the antecedents; give especial attention to their being, or having been, cerebral congestions; and all the precursory symptoms of general palsy must be held in mind to contrast them with our doubtful case. We shall find, if the patient is laboring under general palsy, that his melancholic or hypochondriacal delirium has not only its characteristic absurdity, but occasionally there will be seen, if only momentarily, yet of vast importance, some ambitious ideas, as

of immense wealth, or great fame, gliding into his train of lamentation, fear of death, or a curious state of imagined metamorphosis.

Dr. Aitken nowhere states that the absurd delirium of simple melancholia divulges the complication of ambitious ideas. M. Marcé says : " In simple melancholia the delirium, although monotonous, does not offer the same appearance of absurdity, and never is complicated with ambitious ideas."

In simple melancholia, when the stupor is intense, involuntary evacuations may occur at the very onset, while in general palsy they only slowly and progressively arrive. In melancholia, as soon as there is stupor, there is directly slowness of movement, and muscular weakness, which present a contrast with the energetic, yet stiff and badly co-ordinating movement of the paralytic during the first period. In every doubtful case the only duty, though sometimes annoying, is to delay the diagnosis till something may occur to throw light on the subject.

Not having time to lengthen this part of our subject, we will only mention that many of the inexperienced have often taken for an epileptic attack, apoplectiform or epileptiform congestions caused by general palsy ; that cerebral softening, when situated in the central parts, or occupying both hemispheres, so closely presents symptoms allied to general palsy, that sometimes a *post mortem* has only revealed its true nature. Even in such cases, good authorities state that no ambitious ideas are ever seen.

TREATMENT.

The physician, once feeling sure as to his diagnosis, has an important part to play in regard to the doubts and fears of the patient's relatives and friends. It is useless to here endeavor to indicate how he must comport himself ; only we would remark that this disease opens to him a wide field to employ a large amount of good sound judgment and advice. Let us be cautious not to be led astray, nor allow friends to be misguided by remissions. Notwithstanding we are sure as to the

final result, yet it is our duty to mitigate the patient's sad state as much as is in our power, and ever to seek to obtain those remissions which somewhat resemble the quiet, fertile spot in the arid desert. The mariner, tossing to and fro at the mercy of wind and waves, would gladly hail a calm though he were sure it was to be followed by a tempest that would certainly engulf him.

We must remember that however bad may appear the condition of our patient, yet at the eleventh hour has often come a change, making the victim, as it were, a new man — alas ! to fall again. The possibility of the terrible symptoms disappearing to a great degree, and of this mad destroyer's lulling in its course for a time, should ever be sufficient to encourage the physician, were there nothing else to cheer him on.

As soon as possible, the patient should be removed to some quiet, comfortable, well regulated asylum, where he may be removed from the busy scenes of life, and the cares of his profession. We must counsel friends that the sooner this is done the more chance the patient has ; and the longer it is delayed the more rapid will be his disease ; and that left alone, he will commit, sooner or later, some scandalous or dangerous act at an unexpected moment.

When once the patient is confided to the physician, then he must ever have uppermost in his mind that, though he is not to effectually annihilate its progress and march, yet he now has to work ; as has well remarked M. Calmeil, " Its degree of gravity depends on the extent of cerebral surface the disease occupies, whether in width, length, or depth."

Now must we bring into play all the resources of good hygiene, by furnishing sufficient alimentation, but not too rich in succulent, animal aliment. At an early hour must attention be given to not allowing the circulation to be so active as to produce congestion of the nervous centres. If the patient is robust and plethoric, we may employ local bleeding to a moderate degree by the application of leeches to the nasal fossæ, anus, neck or ears. The cupping glass may be used if more convenient. If we desire to recall an ancient hæmor-

rhoidal flux, or the suppression of the menses, the leech should be applied at the anus or vulva.

If the tendency to cerebral congestion is accompanied with agitation, petulance, or furor, it will be well to often employ baths, which may be prolonged three or four hours, according to necessity. The cold water bath by affusion, the sulphurous bath, and others which prompt reaction of the circulation towards the surface of the body, are preferable to quell the hyperæmic state. During and after the bath, application of cold compresses to the head is strongly recommended.

Continued attention must be given to the alimentary canal; constipation in this disease is constantly present, and also tends to increase congestion. To counteract this latter, the salts of soda and magnesia, and castor oil, are greatly in favor. Jalap alone, or with calomel, is much used.

The use of opium and other narcotics is questionable as to rendering service, and should, if used, be carefully employed. If the disease has assumed the form of melancholia or dementia, then we must be cautious in the employment of bleeding, possibly putting it completely aside; cathartics here, as in other forms, are often necessary. If the patient is troubled with torpor, but is not too much reduced, often the application of a moxa or a cautery is beneficial.

In some cases there is such a degree of anæmia, produced by melancholia, refusal of food, or various excesses, as to require the use of tonic treatment instead of depletion. Cod liver oil, iron, and quinine, then are useful. M. Marcé, at Bicetre — a hospital at Paris — has profitably used iodine and arsenic in small doses, when the digestive organs were inactive.

The activity of the antiphlogistic system should be lessened when the loss of memory, obliteration of the mental faculties, and difficulty of speech continue to increase, as we then see that the malady is attacking new regions in spite of all our endeavors to prevent it.

Now, as the second and third periods have arrived, the cerebral substance being disorganized, all we can do is to admin-

ister to the wants of the patient ; sustain him as much as possible, by hygiene ; give proper attention to the bowels, using all possible means to preserve his person and clothes in a clean condition, thus endeavoring to avoid, as long as possible, the formation of eschars ; if necessary, use the catheter in case of urinary retention ; compel the persons attending to give him such food, in so fine a state, that there shall be no accidents from imperfect mastication and difficult deglutition.

When congestive attacks assume an apoplectiform or epileptiform nature, and there is immediate danger, then must the usual means be employed to dissipate the momentary danger.

From the beginning to the fatal termination, let kindness rule in every word and every action.

EXPERIMENTAL MEDICINE.

Experimental Researches into the Subject of the Action of Phosphorus upon Living Tissues. — Reflections upon the Pathogenesis of Fatty Transformations. By DR. L. RANVIER, Paris.

TRANSLATED EXPRESSLY FOR THE JOURNAL, BY WALTER HAY,
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SECOND SERIES OF EXPERIMENTS.

Experiments 4, 5, and 6.—On the 15th of October, I placed similar bits of *Phosphorus* under the skin of the loins of two other frogs ; I killed one of them on the 23rd, and the other on the 28th. In neither of them were any inflammatory phenomena in the vicinity of the *Phosphorus*. In the first, there were observed yellowish plates and striæ upon the surface and in the interior of the liver ; at these points the hepatic cells were loaded with fatty granulations. In the second the liver was completely degenerated.

A sixth frog was poisoned by the very same process on the first of December, 1866, and exhibited to the Society on the fifteenth of the same month. It presented no inflammatory phenomena in the neighborhood of the foreign body, and the fatty transformation of the liver and kidneys was very complete.

In order to establish the value of these experiments, it was necessary to insert under the skin of different frogs, fragments of inert substance, and to ascertain if they determined around themselves congestions, exudations, or neo-plasmata. This I accomplished in a sufficiently large number of animals, and ascertained that the presence of an inert foreign body, such as a little pebble, or a fragment of thread, placed in the lumbar region of the frogs, produced very speedily congestion, exudation, and even hyperplasia of the conjunctive tissue of the aponeurotic envelope, and of that which accompanies the cutaneous nerves; to such a degree, that at the end of three or four days, the foreign body is completely enveloped by a mass of new formation. Moreover, a variable quantity of serosity accumulated under the skin. None of these phenomena, as has been seen, are produced around a fragment of *Phosphorus*.

Experiment 7.—On the twenty-fourth of September, 1866, I introduced, by means of a sub-cutaneous incision between the ears of a young rabbit, a fragment of *Phosphorus* of seven millimetres in length, by two in width and thickness. Next I removed the os calcis of a new-born rabbit, and placed it in the right flank of the first rabbit. On the following days no inflammatory phenomena supervened, on the side of the phosphorus, whilst at the point where I placed the os calcis I determined the presence of puffiness and tenderness upon pressure. Matters remained in this state until the fourth of October. At this date I killed the animal, and ascertained that the engrafted os calcis was surrounded by a thick, whitish deposit of two to three millimetres in extent, formed of embryonic cells. Sanguineous vessels ramified already in this deposit, which later, as I have determined by other experiments, would have given origin to more perfect tissue. In

the vicinity of the *Phosphorus* nothing similar was recognized; moreover, the *Phosphorus* had preserved its transparency, and its volume was not diminished by an appreciable quantity, whilst the connective tissue circumjacent did not appear to have undergone any modification; it was not hyperæmic, nor infiltrated by exudation. Examined with the microscope, it appears with its fundamental fibrillated substance. These cells at some points were more developed than is usual, but there was no very evident proliferation. The liver, the kidneys, the muscles, had not undergone fatty degeneration, which was probably due to the fact that the toxic substance had not been absorbed in sufficient quantity.

In order to give its entire value to this experiment, I should add, that I introduced into the sub-cutaneous cellular tissue of different rabbits, inert foreign bodies, and constantly established, at the end of a few days, suppurative inflammations.

And if, in this experiment, I introduced under the skin of the animal on one side a fragment of *Phosphorus*, and on the other a portion of living tissue, I did so in order to give the greatest possible value to the experiment, since an animal graft made with care rarely entails suppurative inflammation.

Experiment 8.—The tenth of December, a fragment of *Phosphorus* was placed under the scalp of a Guinea pig, the hair of the region having been carefully cut away.

On the following days no swelling was established, and pressure determined no pain. This condition of affairs existed up to the 24th of December; on that day the animal died accidentally. In the vicinity of the *Phosphorus* there was neither hyperæmia nor exudation; however, the surrounding connective tissue had undergone a slight thickening, and it was determined by the microscope that the number of the cells was notably increased. The different organs presented no alteration.

In this case, in the vicinity of the *Phosphorus* slight evidences of irritation were observed, but they were very much less than those determined by the pressure of inert bodies.

In these different experiments, fragments of *Phospho-*

rus and of other foreign matter were placed simultaneously in the same animal, or in different animals of the same species. In none of these experiments did *Phosphorus*, in its pure state, deposited in the midst of the tissues, and out of contact with the external air, determine around itself any such inflammatory phenomena as those produced by inert matters. Whilst a fragment of *Phosphorus* introduced into living tissue represents, by its persistent, angular form, and by its consistence a veritable foreign substance; it should, like it, determine inflammatory phenomena, if its action as a foreign substance were not counterbalanced by its specific action. This action, which removes from the cells, at least in part, the property of undergoing formative irritation, should therefore be considered as contra-stimulant. It can therefore to-day be no longer admitted that the fatty transformations which supervene in the liver, the kidneys, the muscles, etc., under the influence of *Phosphorus*, are due to the irritant action of this substance. It becomes necessary to resort to other explanations.

There are met with in science three other theories: that of G. Lewin,* which consists in the admission that *Phosphorus* introduced into the digestive canal suppresses entirely the absorption of fat by the chyloferous vessels. The veins would act vicariously for them, and the chyle would penetrate the vena porta, and be carried directly to the liver. The cells of this organ brought into contact with a blood loaded with fat, would become infiltrated therewith. This theory rests upon exact facts.

Indeed, animals to whom phosphoretted oil has been administered, killed from three to four hours after the injection of the toxic substance, have their chyloferous vessels filled with a serous liquid, whilst the portal vein contains blood loaded with fine fatty granulations. It is possible even to administer to the animals *Ether* with the *Phosphoretted oil* without diminishing the transparency of the chyloferous vessels. This

* Lewin, Etudes sur l'empoisonnement par le *Phosphore*, Arch. de Virchow, 2nd Série, t. 1, 1861.

experiment, which I have reproduced many times, demonstrates that the *Phosphorus* has a powerful action in preventing absorption by the lymphatics of the intestine, for, as M. Claude Bernard* has taught us, *Ether* has the remarkable property of stimulating the absorption of fats by the chyliiferous vessels.

This theory of G. Lewin would be satisfactory if the *Phosphorus* introduced into the digestive canal determined fatty transformations in the liver alone, it was formulated by its author at an epoch when it was not known that the fatty transformation involved a great number of organs, might be generalized for all the lymphatics the idea of Lewin concerning the action of *Phosphorus* upon the lymphatics of the intestine, and maintain that if *Phosphorus* determines fatty transformations in different organs, it is because the lymphatics having for their function the resorption of the fat which the elaborated, physiologically, are impeded in this function. But as will be perceived, this would be a substitution, for the hypothesis of Lewin, of another hypothesis in support of which not one fact exists.

The second theory is that of Munk† and Leyden. These two authors having observed that different inorganic acids and certain substances such as *Arsenic* and *Antimony*, as well as *Phosphorus*, determine fatty transformations (polyorganic), were impressed with the idea that these toxic steatoses were the result of the destruction of the red globules of the blood.

In order to demonstrate the inaccuracy of their mode of observation, it suffices to poison frogs with *Phosphorus*, and then, whilst they are still living, to study their circulation by the aid of the microscope. It can thus be established that the red globules which circulate in the capillaries of these animals have undergone no modification in their color or in their form. I have repeated this experiment several times, and it

* Cl. Bernard, *Leçons sur les effets des matières toxiques et médicamenteuses*, 1857.

† Munk and Leyden. *Die aerzte Phosphorvergiftung*, etc. (Rücksicht auf Path. u. Phys., 1865.

has always given me negative results. Moreover, with M. Demonchy we have determined that in frogs poisoned with *Tartar emetic* or *Arsenious acid*, the blood had sustained no morphological alteration; and yet the liver and kidneys of these frogs were in a state of complete fatty transformation.

I have frequently examined the blood of rabbits and cats poisoned by *Phosphorus*, and have never been able to distinguish any alteration of the red globules which could be attributed to the poisoning.

We now come to a third theory, consisting in the claim that phosphorus determines fatty transformations by reason of a specific property. Upon the side of this prudent reservation M. Larcereaux* ranges himself.

In truth, in the present state of science it is difficult to explain the transformations which supervene upon poisoning by *Phosphorus*; but what strikes our attention is this: that amongst toxic substances *Phosphorus* is not the only one which determines fatty degenerescence. It should not therefore be claimed for it that it possesses a specific action, and hence one is induced, by the example of Munk and Leyden, to seek the relations between phosphoric steatoses and other fatty degenerescencies.

This brings me to the second part of this work: to the pathogenesis of fatty transformations, and especially to their relations with the inflammatory process.

We have seen Virchow maintain that inflammation can extend itself into the muscles and into different parenchymata by means of a fatty degenerescence of the histological elements.

In a final stage of neo-formations, inflammatory or otherwise, a fatty transformation of the cells supervenes habitually. Does it follow that this transformation appertains to a developmental movement which characterizes inflammation? Assuredly not. It is seen also in the numerous experiments of Virchow that fatty degenerescence is a process essentially passive. It is then incomprehensible how this illustrious

* Loc. cit.

professor can maintain that in certain cases inflammation, a phenomenon essentially active, could be characterized by fatty transformations.

Facts sufficiently numerous show, moreover, that inflammation and fatty degenerations, in place of being connected in an intimate manner, are, on the contrary, in opposition. In the phlegmon of sub-cutaneous cellular tissue, the adipose cells lose the fat which they contain, their nuclei and the little mass of protoplasm which surrounds them, originate by division to a very abundant production of cells, which fill up the old adipose vesicle. In acute osteitis the adipose medullary is seen to transform itself into embryonic medulla by an identical mechanism.* In acute or chronic arthritis, the cartilaginous cells, which physiologically contain fat, are deprived of it during the time whilst the cellular proliferation persists. This disappearance of the fat in the elements which contain it in a physiological state is met with not only in the inflammatory process, but also in all the active neo-formations. Then, when the neo-plasmata which constitute tumors take their point of departure in the cellular-adipose tissue or in the medulla of bone, they determine the disappearance of the fat in the cells which they involve.

But, a still more important fact: when under the influence of a pathological cause the fatty transformation has invaded certain cells, it is seen that these cells can rid themselves of the fat which they contain under the influence of inflammation, if at any time it should supervene before the cellular elements may have been completely destroyed by degeneration, as results from investigations which I have made into the alterations of diarthrodial cartilages in white swellings.†

However, at the end of the inflammatory process, and in the last phase of every neo-formation, a fatty transformation of the elements, at that time superabundant, is observed.

* Des alterations des cartilages dans les tumeurs blanches. (Bull. de la Soc. Anatom., 1865.

† L. Ranvier, Considération sur le développement du tissu osseux, etc., 1865.

This transformation should not be attributed to an imitative process, but entirely to an alteration of nutrition, for it never supervenes at the moment when the cells are in full proliferation. It happens only at the time that the formative movement is arrested, and that the nutritive exchanges become difficult for the elements whose number is no longer in relation with the vascular development. On the first day of a catarrhal inflammation, the exudation is transparent, and the numerous cells which it contains show themselves with every indication of a very active multiplication, and do not contain a single fatty granulation. Later, when the exudation becomes yellowish and opaque, nearly all the cells enclose granulations, and even some little drops of fat.

It will be remembered that, in the month of September, frogs accumulate fat in their muscles. This fat is probably destined to nourish the animal during hybernation — it is known, indeed, that the frog has no sub-cutaneous adipose cellular tissue — it accumulates fat in the great epiploon and in the muscles. It was interesting to see whether an irritation directed to the fatty muscles of the frog could diminish or remove the fatty granulations, granulations arranged like beads between the elementary fibrillæ. To determine this result, I passed threads through the muscular masses. I effected fractures, and established the fact that from the fifth to the eighth day the fatty granulations had considerably diminished with having completely disappeared in the portion submitted to the irritation. It is probable that this result would be more or less rapid, according to the season of the year, and according to the temperature. My experiments were conducted upon six frogs; four have had the femur fractured; three only were killed from the sixth to the eighth day. In the cases of two of those which had sustained fractures, I awaited the twentieth day, until there had been a commencement of a callus; in these, a limited number of muscular fibres were comprised in the cartilaginous mass, and had undergone complete fatty degeneration. In the last, which was examined twelve days after the experiment, the muscular

bundles in the neighborhood of the thread upon whose passage there had been an abundant cellular neo-formation, had commenced also to undergo fatty degeneration; in these last cases the fatty transformation ought not to be interpreted by the inflammation, but very much rather to the hindrance sustained in the nutrition of the muscular fibres by the presence of tissues of neo-formation between these fasciculi.

I come now to the part the most disputed, and in fact the most difficult of fatty transformations. Whence comes the fat which infiltrates the histological elements? Is it already formed in the blood which merely deposits it in the cells? Does it originate, as there is much evidence to show, from a direct transformation of the albuminoid substance which forms the protoplasma of the cells? Is its accumulation in the cells the result of a deposit or of an exaggerated formation, or, indeed, is not the fat formed physiologically, consumed just in proportion as it is produced? Finally, it may even be asserted that the fatty matters contained in the cells are taken up, little by little, by absorption, and an impediment applied to this function would thence determine a fatty accumulation. It is not my intention to reply to these different questions which have been already disposed of partially, and very imperfectly resolved by Wagner, Mideldorf, Witich, Virchow, etc.; but I desire at present simply to adduce some new evidence. The microscope does not always suffice to detect fat contained in organic liquids or histological elements. Thus in the blood no fatty granulations are recognized, whilst it contains fat in notable quantity. Fatty matters enter into the composition of the red globules in the proportion of 18 to 25 per cent.,* and yet no fatty granulations are to be distinguished in the globules.

These fatty matters, which can not be discovered by the microscope, as is very well established for the red globules of the blood — very probably because they are combined in an intimate manner with other constituent matters, can resume

* Pelonze et Frémy, *Traité de Chimie, générale*, tome VI., 2d édition, p. 100.

their form and characteristic reaction under certain conditions; when the blood has escaped from the vessel, and remains in an accidental receptacle, it is observed that the red globules become decolorized, abandoning their hæmotosine, which is dissolved in the surrounding liquids or concretes in the form of granulations or crystals; moreover, these globules become spherical, lose in the direction of their larger diameter, which falls to five ten thousandths of a millimetre; then is perceived, forming themselves under a membrane which seems to envelop them, some fatty granulations disposed like a string of beads; these granulations are insoluble in acetic acid, and have all the optical characteristics of fat.

In this case I would say that the fat was masked in the globule, and that it had become apparent at the end of chemical transformations as yet badly defined. I will not maintain, with Försher, that a protein substance is transformed into fat.

This theory of fat masked can explain many facts; it is in relation with certain very interesting phenomena which occur in the bodies of foeti which, after having died, have remained from one to three weeks in the uterus.

During the past year I have been able to collect five of these foeti, which I have studied carefully.

In regard to the question which occupies us now, they presented identical modifications, which will obviate the necessity of making special observations upon each one of them.

The blood contained fatty granulations; in four of them the red globules were entirely destroyed; in the fifth a few were recognizable. In the nerve tubes the myelene was fragmentary, and one would have said that the nerves had degenerated after section. In the nervous centres nothing more was found than a semi-fluid mass of fatty granulations, crystals of fat, and cholesterine. The nervous cells alone were intact, and contained no fatty granulations. The liver contained great quantities of fat, and the hepatic cells were destroyed: the epithelial cells of the renal tubuli contained very distinct fatty granulations. The cartilaginous cells of

of the ossific layer, which in the physiological state contain no fat, apparently, at this period of life, contained from one to five fatty granulations. The primitive fasciculi of the muscles of the trunk and of the limbs, the fundamental substance of the cartilages, the cells of the connective tissue, the fundamental substance of this tissue, contained in no case fatty granulations. On the other hand, the muscular fasciculi of the heart were all loaded with it.

From these facts it must be concluded that albuminoid substances do not give origin to the formation of fat when they are abandoned to themselves — that is to say, when they are deprived of life — for if it were not so, it would be impossible to comprehend how the substance of the muscles of the limbs, placed under the same conditions as the muscles of the heart, does not give origin to the formation of fat, whilst in these last, fatty granulations appear. Moreover, the formation of fatty granulations in the cases which we are now discussing is limited actually for each histological element. Some few granulations only show themselves in the cells of the cartilages; they are a little more abundant in the cells of the kidney, they are out of all proportion in the liver; and, finally, in the nervous system, they seem to originate directly from the medullary matter; for the fibres of Remak, so abundant in the foetus, contain here no fatty granulations. I perceive but one mode of explaining these different facts: it is by the assumption that the fatty matters combined more or less feebly with the albumenoid matters are more or less abundant according to the different histological elements. These fatty matters are masked in the different tissues whilst living, and disengage themselves, little by little, after death. It is easy to perceive how interesting would be the results of chemical investigations made in this direction, but I am not yet in a position to give positive results upon this subject. We have, however, only an hypothesis based upon certain facts, and which I hope to demonstrate completely in another work.

It is now apparent why certain elements involved in necrosis might give origin to fatty granulations, without necessi-

tating the intervention of a direct transformation of albuminoid matters into fat. But these fatty granulations would be in this case limited in number; as in the conditions in which much fat originates in the cells. It is necessary, therefore, to seek the aid of a deposit more considerable, a greater elaboration, in default of assimilation or absorption.

If we recur now to the interpretation of fatty transformations in poisoning by *Phosphorus*, resting upon the experiments contained in this memoir, experiments which demonstrate that *Phosphorus* impedes nutrition and the multiplication of cellular elements, to such a degree that the phenomena of inflammation can no more develop themselves, we will understand how these elements can no longer elaborate the fat which they contain in a masked condition, or that which is brought to them by the vascular system.

What corroborates greatly this view of the matter, is that in poisoning by *Phosphorus*, the first organs involved in the fatty degeneration are the liver, the kidneys, and the heart, organs in which fat is recognized in the foetus which has remained dead for some weeks in the cavity of the womb.

CONCLUSIONS.

The protoplasma of the cells appears to be the seat of the exchanges, and of the elaboration of the material carried by the blood; it is likewise in the protoplasma that the fat is first deposited.

The presence of fat in a cell which contains none of it apparent, in a normal state originates from the fact that the nutritive process in the cell is retarded. If this movement is stimulated by irritation, the fat disappears.

Since certain cells manifest great activity in the elaboration of fat, it does not result from this that they form it at the expense of available substances which traverse them; for the blood contains fat in a masked condition — that is to say, in a state of combination or of saponification. The adipose cells, those of the liver, for example, appear to reduce the combined fat to a neutral or insoluble condition.

In the case of the foetus, dead, and remaining for some time in this condition in the uterine cavity, the masked fat becomes apparent to the microscope in the liver, the kidneys, the heart, and the cartilage cells.

Phosphorus determines fatty transformations because it diminishes the nutrition of the histological elements, because it is a contra-stimulant to these elements, a property demonstrated by the experiments detailed in this memoir.*

CASE OF SELF MUTILATION.

BY CURTIS T. FENN, M.D., CHICAGO.

G. S., aged 25, born in New York, a farmer, was admitted to the County Hospital, January 25th, 1868, in a condition of exhaustion. A portion of his clothing was saturated with blood. Examination showed that he had been castrated recently. His wounds were filled with fresh coagulum; and adhering to the clots of blood in his pantaloons, were two rude clamps of green sapling, tied with waxed end. He refused to answer any questions regarding himself; and it was, therefore, surmised that some rural outrage had been thus unmercifully revenged, at the hands of unknown swains. The natural simplicity and innocence of the man, made this

* Since this memoir was drawn up, there has appeared in the Archives de Virchow, a memoir by Dr. Bernhardt upon the alterations of the stomach in poisoning by *Phosphorus*. This author quotes nearly all the observations published in France or Germany, in which the state of the digestive organs of those poisoned fatally by *Phosphorus* had been noted. He adds to it some personal observations, and arrives at this conclusion: that *Phosphorus* does not ordinarily determine inflammation of the gastric mucous membrane.

When this inflammation occurs, it must be attributed to the presence of gas in the stomach at the moment of the ingestion of the *Phosphorus*. This body would then be oxydized, and would occasion the production of *Phosphorus* and phosphoric acids, which alone could act as caustics.

appear doubtful, however. At length, he acknowledged to have committed the act himself; and gave the following history :

He had been inclined to solitude from childhood, and, at times, addicted to self-abuse. He was generally impressed with the idea that he should not be able to rid himself effectually of the habit, till he was castrated. He became afflicted with spermatorrhœa; and, although he felt well and strong, applied to the doctors for a cure, to no purpose. At length, one agreed to perform the operation, at his own request; but required a fee to begin with, which was more money than he could pay. With sublime hardihood, he now resolved to castrate himself, and so to obtain a deliverance which he considered essential to his future happiness. He ground two quadrilateral bits of iron to a cutting edge, and inserted them, an inch apart, into a block of wood. This instrument, he intended for making two separate parallel incisions at once, each to be two inches long. After a month's preparation, all was ready. His procedure then was as follows: For better concealment, and as like to receive more care when he should need it, he came to the city. The morning after his arrival, he seated himself astride an old trunk, in the upper story of his hotel, and contrived to spread his scrotum upon a surface of wood, placed between his thighs, and fasten it there. Taking the implement in one hand, he cautiously and deliberately rested its edges upon the stretched integument, and struck down upon it with a large stone, held in the other; thereby severing all the coverings of both testicles. He then dissected out the cords, and placed upon them rude clamps, he had made the day before. This part of the operation seems to have been performed with less resolution. However, he drew them as tightly together as he could, and, with one stroke of his jack-knife, cut off the offending members. In this condition, he set out for the County Hospital. On getting off the street-car at Eighteenth street, he fainted, and fell to the ground. The police had him taken up, and brought the rest of the way in an express wagon. He was immediately admitted, as an

urgency case, and put to bed. The wounds healed by granulation. He expressed no regret, except at the danger of his exposure. He was discharged, cured, Feb. 17th, 1868.

234 *Thirty-first street, May 23, 1868.*

A CASE OF FRACTURE OF THE VERTEBRA.

BY R. G. BOGUE, M.D., SURGEON TO COOK CO. HOSPITAL, CHICAGO.

JOHN WIDDIS, aged 24, Irish laborer, was admitted into Cook County Hospital, April 14th, 1868. A few hours previous to admission he fell from a hand car while in motion, the car passing over him, quite doubling him up forward. Immediately after the injury, he noticed he had no control over the lower extremities, nor feeling in them.

On admission to hospital there was perfect consciousness, no pain, except at a point in the back, when the body was moved in any direction. At this point, which was in the region of the junction of the dorsal and lumbar vertebræ, there was moderate tumefaction, with tenderness on pressure, very little or no discoloration; a noticeable prominence of a spinous process, and near this, on either side, a hard body could be felt, supposed to be the transverse processes. There was no mobility of any of these prominent points. No other places of injury could be found. There was complete loss of sensation and motion of the lower extremities, the anæsthesia extending to a point about the hips, nearly as high as the cresta ilii. No urine passed since injury.

The diagnosis was: a fracture of the vertebra; and the patient put to bed, lying upon his back, there being apparently nothing to do for him except to minister to his wants in a general way as they should demand.

April 17th.—Patient about the same as when admitted; the bladder has been relieved four or five times a day with catheter; bowels not moved; a large injection has been used,

but no response : three drops of *Croton oil* given, producing two copious evacuations ; appetite fair ; sleeps well ; lies upon the back, and on either side, alternately ; some pain at the point of injury whenever he is turned in bed. The prominent points, where pressure comes, begun soon to look a little red, as though they might slough.

24th.—Has remained much the same, except failed in strength and flesh ; bed-sores have begun to form over the sacrum and trochanters ; no pain ; bowels moved by cathartics or enemata ; urine drawn by catheter. There is now marked deformity at the point of injury, the spinous and transverse processes being plainly made out.

May 9th.—Has gradually failed, as above ; bed sores rapidly becoming larger, and an extensive burrowing abscess has formed in one thigh and leg, discharging a large quantity of thin, offensive pus ; sloughs have formed about the knees and ankles. The bed sores were dressed with *Permanganate of Potash* until the sloughs separated, then with *Carbolic acid*. Thin, fecal matter leaks away from the bowels almost constantly ; urine dribbles away ; bladder not distended.

He has had one terrible paroxysm of pain, referred to the testicles, lasting an hour or so, relieved by large and repeated doses of *Morphine*, and warm fomentation to the parts. He was early put upon a water bed, but the bed sores continued to increase.

May 14th.—He is a good deal more prostrated and emaciated ; a irritative or hectic fever has begun ; pulse small, frequent and feeble, but little appetite, diarrhoea, sweats, sordes about the teeth, and low, muttering delirium. This condition continued until the morning of May 31st, when he died exhausted. He has had another severe paroxysm of pain in the testicles.

His mind remained perfectly clear until about one week before death, complained of no pain except the paroxysms above mentioned. His nourishment was liberal, and ale allowed. The temperature was noted repeatedly, being about two degrees higher below the injury than above it.

Post-mortem examination made about eight hours after death. The body very much emaciated, large and numerous bed sores upon the hips, and lower extremities; a marked deformity at the point of injury in the back. A section of the vertebral column was removed, revealing a fracture from below upward and backward of the body of the twelfth dorsal vertebra, and through the upper and outer part of the base of the transverse processes, and the ligamentous between the eleventh and twelfth dorsal spinous processes torn, the upper fragment, with the vertebræ above, shoved forward and downward, producing complete compression of the cord against the upper and posterior part of the lower fragment; the last ribs were partially torn from the vertebræ at their articulation, and twisted downward. The cord, about the point of compression was softened, the meninges adherent to the wall of the canal; in and about the injured parts there was a good deal of irrepairative material, a part of which had become bony; most of the inter-vertebral cartilage seems to have been destroyed by pressure of the fragments, or by absorption: a fragment of bone was separated and thrust upward between the vertebræ and the meninges.

RUSH MEDICAL COLLEGE. — SPRING COURSE
OF LECTURES.

PROF. ALLEN: — Allow us, in behalf of the class in attendance at the Spring Course of Lectures at Rush Medical College, to express, through the columns of the JOURNAL: first, our high estimate of the opportunities for study so generously afforded medical students, by the faculty of the above-named institution; second, our thorough appreciation of the course of Lectures delivered by Dr. C. T. Parkes, on Comparative and Descriptive Anatomy; Prof. Blaney and Dr. I. N. Danforth, on Toxicology and Medical Jurisprudence; Prof. Freer, on Physiology and Microscopic Anatomy; Dr. Fenn, on Ob-

stetrics and Diseases of Women and Children; Dr. J. E. Owen, on Surgery; and Dr. Wm. C. Lyman, on Surgery and Diseases of the Chest. We have also witnessed, with much pleasure and profit, Prof. Gunn's numerous and very successful operations at the weekly surgical Clinic; Prof. Powell's vivisections, illustrating the course on Toxicology; and Dr. W. R. Marsh's careful examination of cases presented at the medical Clinic. We are also under obligations for opportunities for extended observation at the County and City Hospital, directed, in the surgical department by Dr. R. G. Bogue, and in the medical by Dr. Thomas Bevan; and for similar favors at the Eye and Ear Infirmary, under the direction of Prof. E. L. Holmes.

These gentlemen have labored with unmistakable interest in behalf of the class, leaving no available means unused to make the Spring Course of Lectures, just closed, pleasant and profitable; and we hereby tender them, each and all, our thanks, with assurances of high personal esteem.

During the Spring Term, Prof. Blaney has conducted, most successfully, a course of instruction in Practical Chemistry, affording a rare opportunity for the acquisition of most useful knowledge — the possession of which demonstrates to every student the beauties of chemical science, together with its positive character, which can only be fully comprehended after practical manipulations in the laboratory. A large proportion of the class have attended this course, and desire us to say that they are a unit in their hearty appreciation of the benefits thus derived, and also in their appreciation of Prof. Blaney as a teacher of chemistry.

All things considered, we deem this Spring Course of great value to us as medical students, furnishing opportunities for observation and instruction not attainable in the more formal regular course; and we shall ever be thankful for its thoroughly taught lessons and pleasant associations.

F. L. WADSWORTH, }
M. DONNELLY, } *Committee.*
R. BROUGHTON, }

CHICAGO, ILL., *May 30th*, 1868.

CORRESPONDENCE.

"There is nothing new under the sun." — SOLOMON.

"We travel in circles." — GEN. SHERMAN.

MORE particularly were these axioms of wise and great men brought to my mind, by *two very recent discoveries*, and their truth demonstrated.

Very recently, in the western part of New York, is the claim for reduction of dislocated femur, by tact, without pulleys or traction — or, in other words, make the muscles restore the integrity of the joint, without force or suffering — made by a surgeon; and, *mirabile dictu*, one and another claimed the discovery, each giving dates and facts to show precedence and originality. This is *singular*, and can only be accounted for by the axioms above.

In 1832, I graduated at the University of Maryland, in Baltimore city, and wrote my thesis "Upon the American Method of Reducing Dislocated Femurs." Prof. N. R. Smith (and may his shadow never grow less!) lectured, as he does now, on surgery, and gave us the history of cases reduced by his father, Prof. Nathan Smith, of Yale; exhibited drawings on the subject, and demonstrated the success of the plan. In compliment to the father, from love of the son, I wrote a thesis; had all of Cooper's plates of pulleys, and other tortures, attached, and demonstrated, by plates and drawings, the feasibility and success of the operation; and gave the credit of discovery to the elder Smith. I shall never forget standing in the presence of the graduating class and two thousand persons, to defend this thesis by seeming attacks of the son, and, on a black-board, in the heat of the controversy, demonstrated, as I thought, as fully as any one of the problems of Euclid, the feasibility of the plan, and the truth of its being an American discovery, and Nathan Smith the author. Yet, within

five years, there has been quite a controversy between two or three comparatively young men, each claiming and proving, by time (that can not lie), to be the discoverers. "There is nothing new," etc.

What brought this subject most to my mind was, in reading a "volume of forgotten lore," the article on "Fever, Malarial, Cryptogamous, Origin of; by Dr. J. K. Mitchell, of Philadelphia; Published in 1849." Whatever may be said of the truth or fallacy of the opinions given in the article, one thing is true: in certain lectures delivered before then, the opinions were asserted, and founded on certain facts and hypotheses; and had not Dr. Cowdell's Essay claimed this as a discovery, Dr. Mitchell never would have more than simply asserted, in his lectures, these facts as original. He published, and fully (as he thought) vindicated, not only the originality of the discovery, but its truth.

Dr. Cowdell is not the only claimant. Either Prof. Saulsberry, of Cleveland, or his friends here for him, claim that he is the author of this most true and most important discovery, the cryptogamous origin of fever; and many of the experiments were made here, and were observed and attested by several of our most talented physicians, very recently.

As we have remarked, the facts were set forth in lectures; and, in 1849, Dr. Mitchell issued a book, contesting the claim of Dr. Cowdell. Of course, any thing since will give way to the facts then published; and, in order to give your readers the facts, with your permission, I will copy Dr. Mitchell's remarks:

"My preference is founded on the vast number, extraordinary variety, minuteness, diffusion, and climatic peculiarity, of the fungi. The pores of these plants are not only numerous, minute, and indefinitely diffused, but they are so like to animal cells, as to have the power of penetrating into and germinating upon the most interior tissues of the human body. Introduced into the body through the stomach, or by the skin or lungs, cryptogamous poisons were shown to produce diseases of a febrile character, intermittent, remittent and contin-

ned, which were most successfully treated by wine and bark. As microscopic investigations become more minute, we find protophytes in disease, where, until our own time, their existence was not even suspected; as in the discharges of some kinds of dysentery, and in the *sarcini* of pyrosis. We are, therefore, entitled to believe that discovery will be, on this subject, progressive. The detection of the origin of the muscardine of the silk-worm, and a great many analogous diseases of insects, fishes and reptiles — and the demonstration of the cryptogamism of these maladies, their contagious character in one species of animals, their transfer to many other species, nay, even to vegetables themselves — all concur to render less improbable the agency of fungi in the causation of diseases of a febrile character. A curious citation was subsequently made of the fungiferous condition during epidemics and epizotics. These moulds, red, white, yellow, grey, or even black, stained garments, utensils, and pavements, made the fogs foetid, and caused disagreeable odors and spots, even in the recesses of closets, and the interior of trunks and desks. These moulds existed even when the hygromatic state did not give to the air any unusual moisture for their sustenance and propagation. Their genus seemed to have, as many epidemics have, an inherent power of extension. The singular prevalence of malarious diseases in the autumn, is best explained by supposing them to be produced by the fungi, which grow most commonly at that season. The season of greatest protophytic activity is, in every country, the period of greatest malarious disturbance. The sickly season is in the rains of Africa, in the very dry season in Majorca and Sardinia, in the rainy season in the insular West Indies, and in the dry season of Demarara and Surinam. Even where the vegetation is peculiarly controlled, as in Egypt by the Nile, and the cryptogami are thus thrown into the season of winter and spring, that season becomes, contrary to rule, the pestilential part of the year. Marshes are a safe residence by day, while they are often highly dangerous by night. In the most deadly localities of our Southern country and of Africa, the sportsman may tread

the mass of a swamp by day, although at every step he extricates vast quantities of the gases, which lie entangled in mud and vegetable mould. This point, so readily explained by reference to the acknowledged nocturnal growth and power of the fungi, is a complete stumbling-block to the miasmatists. The cryptogamous theory well explains the obstruction to the progress of malaria offered by a road, a wall, a screen of trees, a veil, or a gauze curtain. It also accounts for the localization of an ague, a yellow fever, or cholera; and the want of power in steady winds to convey malarious diseases into the heart of a city, from the adjacent country. It explains, also, well, the security afforded by artificially drying the air of malarious places, the exemption of cooks and smiths from the sweating sickness, the cause of the danger from mouldy sheets, and of the stercoration from old books and papers. On no other theory can we so well account, if account at all, for the phenomena of milkybrand and milk-sickness, the introduction of yellow fever into northern ports, and the wonderful irregularities of the progress of cholera. The cryptogamous theory will well explain the domestications of different diseases, in different regions which have a similar climate—the plague of Egypt, the yellow fever of the Antilles, and the cholera of India. It accounts, too, for their occasional expansion into unaccustomed places, and their retreat back to their original haunts. Our hypothesis will also enable us to tell why malarious sickness is disproportionate to the character of the seasons; why it infests some tropical countries, and spares others; why the dry Maremma abounds with fevers, while the wet shores of Brazil and Australia actually luxuriate in healthfulness. The prolonged incubative period, the frequent relapses of intermittents, and the latency of the malarious poisons for months, can only be well explained by adopting the theory of a fungous causation. Finally, it explains the cause of the non-concurrence of very potent maladies, better than the chemical theory of Liebig; and shows why the earliest causes of an epidemic are commonly the most fatal.” (Pp. 33–7.)

I have copied thus far, to settle priority of the claim to this

theory, whether true or false; and to prove "We travel in circles."

When Vasco de Gama sailed round the Cape of Good Hope, in 1497, and when Venice lost her supremacy, and commercial prosperity dawned on Europe and England, he thought he was the first to circumnavigate Africa; but he was mistaken. He but followed Pharoah-necho, the Egyptian king, who traversed the same, two thousand years before. And now, when the world is startled at the greatness and usefulness of the canal made by the present Napoleon, connecting the Red with the Mediterranean Sea, he is only doing what was *twice* before done — and once in a greater canal than his, connecting the Nile with the Red Sea. I could multiply cases, in the discovery of brass and glass; but I only wish to prove that "There is nothing new under the sun," and that "We travel in circles;" and to demonstrate that many great discoveries are but as "Charlie's," known to all men, but really believed discoveries by their authors.

Equally as bad, as claiming other discoveries — and worse, if possible — was our Government adopting some Frenchman's great antidote for snake-bite, putting it among articles furnished to army surgeons; — a discovery of Prof. Brainard. He told me, after years of experiment with snakes in his office, and his friends abusing him roundly for their presence and fœtor, he discovered a remedy as antidotal to the poison of snakes; and, on publishing it, the Government had put up the identical prescription, under the name of *Bibron's Antidote*, and sends it even now, after he made the full explanation of the facts to the War Department. This is almost as unpleasant "as a bad picture, and worse bust." E. O. T.

Cubebic Acid.

The active principle of *Cubebs* has been found to reside in *Cubebic acid*, a chrystallizable constituent, and not in the volatile oil or resins. From eight to thirty grains of this, in pill, in the twenty-four hours, will, it is said, cure in six days. Mild astringent injections will soon remove any remaining symptoms, in the less tractable cases.

EDITORIAL.

Items, News, and Gossip.

THE Supreme Court of Michigan "reserves its decision" on the question whether the "vinegar factory" established at Grand Rapids by the Board of Regents of the University at Ann Arbor is or is not a *Department*, in the eye of the law, of the latter concern. It is *still* in abeyance. Several years ago it will be recollected the same court was "equally divided in its combined mind on the Prohibitory Liquor Law." The old question now comes up in a new shape.—The resignations of Profs. Armor and Greene have been accepted. Prof. Sager still doubts, and Prof. Ford is at present in very deep water.—The best essay on Medical Education we have recently read we reproduce bodily from the *Medical Gazette*:—"Many of our exchanges discuss the question of medical education from various points of view. It seems to us that there are two indications to be fulfilled; first, to select students with enough brain-room to contain the necessary amount of knowledge, and second, to fill that brain-room with proper material. The difficulty lies rather with the learners than with the teachers in every profession. It is impossible to get a pint of cream into a gill measure."—To which, however, we add: It is as difficult to get cream from some *cattle* as "blood from a turnip."—Liebig's artificial milk is an imitation of the natural milk, as near as chemistry can make it. Boil $\frac{1}{2}$ oz. of wheat flour to a paste in 5 oz. of skimmed milk. Then add $\frac{1}{2}$ oz. bruised malt, 1 oz. water, and a solution of 3 grammes (about 20 grains) of bicarb. pot. in 11 parts water. Keep this in a bottle surrounded with warm water till it gets creamy. Then put it on a fire, and then strain it through a fine sieve.—Prof. Robley Dunglison has resigned the position he has long and illustriously filled in Jefferson Medical College.—Clarke's process of preserving

bodies, mainly by carbolic acid, proves very successful. At Bellevue an autopsy was made, and the tissues found perfectly fresh, although 107 days after death. We shall publish the process next number.

WE trust some of our readers will give us the results of their observations on the pernicious effects of the too long continued and excessive use of *Bromide of Potassium*. We have seen those of very disastrous character, but our profound regard for the code of ethics temporarily obliges us to abstain from details. —A little *Ether* in the bottle containing *Ergot* or similar substances, liable to destruction by insects, will be found a good preservative. —The *Pacific Medical and Surgical Journal* now reaches our sanctum regularly, and we take pleasure in saying, is one of our most valued and valuable exchanges. It is published at San Francisco by Bancroft & Co., and edited by the Drs. Gibbons. \$5.00 a year, postage prepaid.

THE ILLINOIS STATE DENTAL SOCIETY held its fifth annual meeting at Springfield, May 12, 1868. At the last meeting of this Society, Drs. French and Smith were appointed to present subjects for discussion. The topics embraced Facial Neuralgia, Operative Dentistry, Treatment of Decayed Six Year Molars, Plugging Pulp Cavities and Canals, Receding of Gums, etc., Local and General Anæsthesia, Vulcanite, Gold Plates, and miscellaneous business. We have been informed that the diligent members of this Society had a well attended and instructive session. The subjects were reviewed fully, and much light shed upon the details of the science.

The New York *Medical Gazette* passes under the control of A. L. Carroll, M.D. His salutatory is brief and to the point:

THE most notable event of the week, as regards this periodical, has been the shifting of its editorial responsibilities upon new shoulders; a transition effected with such volcanic rapidity that its strata may be found, perhaps, somewhat distorted. We therefore crave our readers' indulgence for any plutonic ruggedness of aspect in our literary landscape this week, and hope, hereafter, to smoothe its surface with fertile deposit.

We have authority for stating that Dr. Duffield's *Vacuomaceration Process* is not patented. A full description is given on page 275 of April 15th No. of *MEDICAL JOURNAL*.

North-western Medical Agency.

The *JOURNAL* takes pleasure in directing attention to the advertisement of Messrs. Walker & Son, on our advertising

pages. The profession will find them thoroughly competent and reliable. Orders of all kinds may be entrusted to them with entire confidence that they will be attended with promptness, sound judgment, and at reasonable rates.

H. V. PASSAGE, M.D., of Peru, Ind., in a private note, says: "Mr. Stohlman, of the firm of Tieman & Co., N.Y., made the first hypodermic syringe at the request, and for the use, of the late Prof. Brainard, *over twenty years ago*. The French condemned the instrument when it was exhibited to them and its uses explained by Dr. B., but long afterwards claimed it as a specimen of French invention."

THE prospects of *Rush Medical College* were never more flattering than at the present. The applications for circulars and letters are multitudinous from every part of the country. The present Editor thinks this is because the profession recognize the fact that its Faculty attend to their business as such, and do not meddle with *isms* and Utopian schemes hatched in empty heads. The best kind of *reform* begins at home.

CORRESPONDENTS, we trust, we trust, will still bear with us. Their favors are duly appreciated, and will appear as space permits. LOOT will be afforded in abundance shortly.

Cause of Scurvy.

Recent experiments upon frogs, dogs, and other animals, by M. Prussak, of St. Petersburg, tend to sustain the older view that scurvy is produced by an excess of common salt in the blood, occasioned by an exclusive salt meat diet. Injections of salt water beneath the skin of frogs, caused the blood corpuscles to distend the vessels, and gave rise to patches of dark colored extravasations, very like scorbutic blotches. It is suggested, in explanation, that excessive osmosis occurs in consequence of the increased density of the blood.

New Remedy in Intermittent Fever.

A correspondent of the *Southern Journal of Medical Science*, says that he has been successful in curing several cases of obstinate intermittent fever, where *Quinine* had failed, by using the *Liquor Ferri Persulphatis*. He usually premised a full dose of *Pil. cath. co.*, and then gave the *Liq. fer. p.* in doses of from eight to fifteen drops every four or six hours.